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MELIORATION AND SELF-EXPERIMENTATION

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Operant researchers rarely use the arena of applied psychology to motivate or to judge their research. Absence of tests by application weakens the field of basic operant research. Early in their development, the physical and biological sciences emphasized meliorative aspects of research. Improvement of human life was a major goal of these young sciences. This paper argues that if basic operant researchers analogously invoked a melioration criterion, the operant field might avoid its tendency toward ingrowth and instead generate a broadly influential science. Operant researchers could incorporate melioration by (a) creating animal models to study applied problems; (b) confronting questions raised by applied analysts and testing hypotheses in applied settings; or (c) performing self-experiments—that is, using experimental methods and behavioral techniques to study and change the experimenter's behavior.

Key words: melioration, self-experimentation, randomness, basic vs. applied research, human subjects

A journey of one thousand miles begins with a single step. But explorers must consider carefully their initial direction if they would reach their goals. Today, few question the powerful results of the directions taken by biology, chemistry, or physics. Each of these sciences has developed a large corpus of facts and theories, and each contributes to society through associated applied fields, thereby demonstrating its validity. In the "almost science" of psychology (Monte, 1975), perhaps because the complexity of its subject matter has caused such a long adolescence, one may still wonder about the direction of the "field." Psychology is divided into subfields, but unlike the divisions in the more mature sciences, workers in these subfields often maintain that theirs is *the* correct model for confronting all important psychological questions. Thus, behaviorists such as Skinner have argued forcefully for their procedures, but nonbehaviorists, such as Piaget, Lorenz, or Chomsky, have argued equally forcefully. A question, then, is how might a disinterested observer choose between the subfields; how might one choose between various lines of research within a given subfield, for example, involving operant boxes versus mazes; or how might researchers judge their own work so as to decide when to persist and when to change?

The main question of this paper is whether the subfield of basic behavior-analytic, or operant, research can set the direction for a functional and viable science of psychology and, if not, how might that direction be improved. The field of basic behavior-analytic research is active and productive, as indicated by participation in the annual meetings of the Association for Behavior Analysis and other regional, national, and international meetings, and by the voluminous publications by operant researchers in journals such as *Journal of the Experimental Analysis of Behavior, Animal Learning & Behavior,* and *Journal of Experimental Psychology: Animal Behavior Processes,* as well as in numerous recent research compendia, such as those edited by Honig and Staddon (1977), Zeiler and Harzem (1979), Staddon (1980a), Harzem and Zeiler (1981), Commons and

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Nevin (1981), and Commons, Herrnstein, and Rachlin (1982). The field is vigorous.

Powerful reinforcers maintain these repertoires of persistent scientific behavior and Skinner (1959) has identified some. Operant research is relatively easy to perform: it is automated; efficient techniques have been worked out with respect to, for example, deprivation levels, reinforcement quality and quantity, and the force necessary to operate pigeon keys or rat levers; researchers can buy ready-made chambers and programming apparatus; and there exist obvious research gaps to be filled with potential Ph.D theses, publications, or grant proposals. Skinner (1959) pointed to the orderliness, or lawfulness, of operant data as another reinforcer. Cumulative records of rates of responding yield systematic curves. Furthermore, it is "fun" to do basic operant research—certainly it continues to be fun for me and many of my colleagues: to observe schedule effects, to predict response rates or choices, and to ask our animal friends puzzling questions.

However, these important considerations by themselves do not show that the early direction of psychology has been or can be productively guided by basic behavior-analytic research. Although the fun of that research maintains the scientist's behavior, other disciplines and activities provide at least as much enjoyment. Joy may be necessary but not sufficient for defining a productive line of research. Second, orderliness of data is important and it is commendable that so many behavior-analytic experiments produce orderly results, but the meaningfulness or external validity of a curve is at least as important as how well the points fit the curve. Data can be orderly and irrelevant. (It may be of interest to note that a number of productive Harvard psychologists have emphasized orderly data and simple functions, while perhaps slighting other dimensions of research. S. S. Stevens once told me that he left the field of animal learning because of the complexity and disarray of the data. He subsequently spent much of his scientific career demonstrating the generality of the power function, which plots as a straight line on log-log coordinates, to describe psychophysical data. Skinner [1984] continues to emphasize the orderliness of his data. And R. J. Herrnstein and his students have spent more than 20 years studying concurrent schedules of reinforcement with changeover delays, largely because relative response rates plot as a linear function of relative reinforcement rates.) The third reinforcing aspect of operant research, its ease, may be desirable but is not known to be related to significance.

Basic research of the type published in the *Journal of the Experimental Analysis of Behavior* has had relatively little influence on society and little effect on most of our lives. (Compare the influence on the average person's daily life of the biological and physical sciences with the influence of operant psychology.) That is perhaps the most important reason to question the role of operant psychology as guide for the science of psychology.

Some argue that it is impossible to judge the ultimate contribution to society of any contemporary research. I agree that judgment is difficult. Research often meanders, often seems to revolve in inconsequential circles, or is not immediately useful. A scientific goal, like goals in many competitive sports, is sometimes best attained by not rushing directly towards it. Many of the most important results in science have been found serendipitously. For example, the discoveries by Galvani of current electricity, by Pasteur of the principle of immunization, by von Roentgen that x-rays penetrate opaque materials, and by Fleming of penicillin were all largely serendipitous. Well known phenomena in psychology were also discovered through adventitious observations: two of the most noteworthy are Skinner's original observations in operant psychology and James Olds' observations leading to the study of intracranial reinforcement. However, the fact that successful science sometimes meanders does not imply that all scientific meanderings are equally valuable. Phrenology, widely accepted for a time, did not turn out to be particularly helpful, and some would say that Hullian learning theory, too, ended in a cul-de-sac.

The lack of influence of operant research may be due in part to its oft-noted ingrown nature. Recent developments in the field suggest a broadening: operant conditioning research now involves economic theory (Hursh, 1980; Rachlin, Green, Kagel, & Battalio, 1976); ecological theory (Collier, 1982; Shettleworth, 1975; Staddon, 1980b), and cognitive theory (Hulse, Fowler, & Honig, 1978; Roitblat, 1982). Also, there have been recent attempts to relate basic operant findings to applied domains (e.g., Myerson & Hale, in press). But in general the field has been narrow, and the basic research questions studied for the past 50 years have been of little interest to other than operant conditioners. Studies of basic schedules of reinforcement have continually explored relatively few responses—key pecks of pigeons and bar presses of rats being most common—and few reinforcers; and studies of animals' choices have used a narrowly defined method, typically that of the concurrent or concurrent-chain schedule. These examples can be amplified, but the point is that most contemporary operant research appears to derive mainly from long-established operant *methods*. The goal of such research increasingly appears to be the creation of mathematical models that describe the responses observed under these methods. Most popular areas of operant work—on simple, multiple, and concurrent schedules of reinforcement—refer only rarely to other fields of psychology or to questions raised outside the operant laboratory. Similarly, other fields, notably applied behavior analysis, make little or no reference to basic operant research (see Poling, Picker, Grossett, Hall-Johnson, & Holbrook, 1981). A number of critics (e.g., Gleitman, 1981) have remarked that even behavior modification procedures have, in fact, derived little from basic operant conditioning research. We shall return to these points.

Missing from the melange of criteria now guiding basic operant research is melioration. Whether or not research is meliorative depends upon its effects on domains other than the research itself: What outside-the-laboratory problems does the research confront? What improvement will this particular research directly engender? How does the research help people or the community? If evaluated according to melioration, basic operant research might break from its narrow confines. A second reason for a melioration criterion may be unique to our times. We live in a time of increasing threats to survival. Unless our activities are more effective in dealing with these threats, none of us will be able to do anything else. Simply put, basic scientists may not have the time to meander (see Nevin, 1982). We meliorate or face extinction.

A possible argument against melioration is that emphasizing it delays the groundwork research necessary for establishing the basic science, and science ultimately is the most certain route to melioration. If true, that would be a powerful argument. However, some notable historians argue that the early history of the physical sciences indicates quite the contrary. Modern science began with an assumption that science must help people.

> Scientific research and publication of the findings [during the Renaissance were] . . . clearly recognized as a service to the public and prerequisites of progress . . . [and Francis Bacon] proclaimed the advancement of knowledge for the benefit of mankind as the goal of the scientist. (Zilsel, 1957, pp. 264, 259)

The Renaissance scientist's emphasis on practical application is to be contrasted with less successful earlier attempts at scientific activities: Although the ancients, from pre-Socratic times through the 3rd century A.D., anticipated many of the practices and techniques of modern science, there was a general lack of appreciation for application as a major goal of scientific work. Rather, the ancient scientist was concerned with understanding an already perfect world that needed no improvement. Ludwig Edelstein

(1957) contrasted the ancient and Renaissance scientists as follows:

> As for the [ancient] scientist, . . . his true reward remained the insight he had gained into nature. The "sober drunkenness" of cognition was his highest goal. No ancient scientist, I think, could have said what Pasteur said of himself: "To him who devotes his life to science, nothing can give more happiness than increasing the number of discoveries, but his cup of joy is full when the results of his studies immediately find practical applications." It is rather in the words of Ptolemy that the happiness and joy of the ancient scientist is summarized: "I know that I am mortal, a creature of a day; but when I search into the multitudinous revolving spirals of the stars, my feet no longer rest on the earth, but, standing by Zeus himself, I take my fill of ambrosia, the food of the gods." His own inner enrichment constituted the celebrated humanism of the ancient scientist. He tried to acquire knowledge for knowledge's sake. . . . The artisans of the Renaissance [on the other hand] . . . justified their inventions by reference . . . to the usefulness of their craft and the public benefit. No ancient artisan could have thought of such a justification. (pp. 100-101)

Usefulness, application, and melioration are not magic potions, but especially at the early stages of a science, before its path, methods, and domain have clearly been established, melioration may serve to distinguish fruitful and potentially enduring lines of research from dead-end labors. As the science matures, melioration may become a relatively less important guide while other criteria such as order, theoretical breadth, and aesthetic and intuitive appeal are added (Beveridge, 1957). The ancient versus Renaissance attempts at science are especially relevant to psychology today, for then, as now, there were conflicting methods, antagonistic philosophies, and multiple claimants to the true science. I do not argue against basic research but rather against basic research that, *at an early stage of its development*, ignores a melioration criterion.

Basic behavior-analytic science is fortunate in its historical association with the field of applied behavior analysis. B. F. Skinner was, of course, instrumental in establishing both fields, and Skinner himself, as well as many of his colleagues (e.g., F. Keller, C. Ferster, and N. Azrin) have divided their professional careers between basic and applied studies and writings. However, as noted above, even a cursory review of *Journal of the Experimental Analysis of Behavior* (JEAB), the basic research journal, and *Journal of Applied Behavior Analysis* (JABA), the applied journal, shows that there is little overlap in subject matter and little cross-referencing. Although basic operant research and applied behavior analysis share common Skinnerian ancestry, language, and, at least in part, philosophy, the questions confronted appear to be quite distinct.

Furthermore, to the extent that there *is* interaction between the two fields, it is primarily unidirectional—from basic to applied. The applied analyst sometimes uses contingencies originally explored in the basic operant laboratory, and often uses operant language. But rarely does the basic researcher profit from the toils of the applier. If the early histories of our sister sciences are relevant, then cross-fertilization would benefit both basic and applied research. The basic researcher would provide the applied analyst with phenomena, theories, and techniques; the applied analyst would provide the basic researcher with an arena for testing potential applications of the basic researcher's work. The basic researcher's findings would continually be tested by the questions: Does it work? and Is it useful? Applied tests would help the behavior-analytic field avoid self-contained, and possibly barren, areas. Some questions would naturally demand study outside the traditional experimental chamber. For example, if a major concern is how best to influence or control another organism, then it might be best to study the behavior of successful politicians, or of priests and other persons of religion, or of successful animal trainers. The basic behavioral researcher might enter that strange world of hypnosis, where an extraordinary form of behavioral influence is manifested. (There are fascinating parallels between the processes of hypnotic induction and operant shaping.) In brief, a melioration criterion might change basic behavioral research from its present methodological orientation to problem orientation. B. F. Skinner's inventions would then provide not only the securing anchor of a method, but a model of helpful inventiveness. On the other hand, if the test of application is ignored, operant conditioning research may not become an influential science.

There are many ways in which basic operant-conditioning researchers could utilize an applications criterion. One is through experimenting with animal models in ways explicitly designed to confront extra-laboratory questions or problems. For example, Masserman (1943) attempted to provide an experimental basis for psychoanalytic therapy; Seligman's (1975) learned-helplessness research was immediately related to, and tested in, the human arena; a number of studies have attempted to model human phobias with rats, and to test different ways to overcome such phobias (e.g., Baum, 1970; Katzev & Berman, 1974); a variety of studies with pigeons (e.g., Rachlin & Green, 1972; Navarick & Fantino, 1976) have been related to human self-control; and, in some cases, explicit testable predictions about human self-control were made from the animal model (Grosch & Neuringer, 1981). Thus, through their choices of models and paradigms, basic operant researchers can orient their research to confront extra-laboratory issues.

Alternatively, basic research would become more relevant to extra-laboratory questions if the research were performed on the subject of ultimate concern. Most JABA research is of this kind. The subjects of study are often themselves the object of concern. But JABA research is typically directed toward assessing the efficacy of a single, immediate application rather than exploring basic issues.

Self-experimentation, where researcher and subject are one, provides another alternative. The self-experimenter, whether physician, chemist, or psychologist, uses him or herself as primary subject in an otherwise "normal" experimental program, this being done sometimes for ethical reasons (e.g., the physician may not want to subject others to the possible dangers of a new drug) or for methodological reasons (e.g., to study "private" phenomena). There is a long, although sparse, history of research done by self-experimenters, in psychology as well as other sciences, on both basic and applied questions (see Altman, 1972; Neuringer, 1981). For example, Ebbinghaus (1913) did most of his seminal memory research on himself as the only subject; Stratton (1897) wore inverted lenses and discovered the rapid adaptation of human perceptual systems (see also Kohler, 1962); and E. G. Boring (1915) inserted tubes into his alimentary canal to study his own internal sensations.

By its nature, self-experimentation tends to be meliorative, or relevant to one's daily concerns, for self-experimenters are hesitant to subject themselves to research that has no relevance. Thus, for example, I performed a series of experiments on the effects of exercise on learning and problem-solving (Neuringer, 1981). The research was partly motivated by my apparent inability to sit still whenever I engaged in serious thinking, and by my interaction with two colleagues who advised activity. Among other things, I found that I learned more rapidly after running 2 miles than after an equal period of sitting at a desk. During that research, I did a brief series of informal A-B-A trials in which I sometimes ran when I had a headache and other times did not. With high probability, the headache would disappear only after I ran. Swimming for 20 to 30 minutes provided similar relief for me. It is no accident that I studied learning and headaches, two topics dear to me. My students have chosen differently. For example, one student, a long-distance runner with poor vision, examined the effects on visual acuity of running 6 miles. She had theoretical reasons to hypothesize (a) that relaxation improves acuity, and (b) that running increases relaxation. She found a consistent, although small, improvement in her visual acuity immediately following the exercise. However, a parametric study of the duration of the effect showed that the improvement lasted less than an hour after the exercise, a short-lived effect indeed.

The results of self-research are not all positive. For example, another student, committed to Transcendental Meditation and its beneficial effects on all aspects of life, explored his learning and memory as a function of periods of meditating versus control periods when he did not meditate. He found no differences. The point, simply, is that expectations or preferences of self-experimenters do not necessarily lead to the expected and desired outcome. But it is usually the case that a self-experimenter studies questions of immediate relevance to him or herself.

Self-experimentation can be used to study basic issues of philosophical or ethical importance to the self-experimenter. For example, there is conflict within Skinner's writings regarding the determined versus indeterminate nature of operant responses. Clearly, according to him, the operant is not "caused" or determined in a Newtonian sense—that is, no one-to-one correlation exists between prior environmental event and operant response (e.g., Skinner, 1938, pp. 19-21). However, Skinner also argues that as we gain increasing knowledge about the laws of behavior, we will be better able to predict and control behavior (Skinner, 1971). (One should not fault Skinner for an ambivalence here; some of history's most incisive thinkers shared this ambivalence towards "free versus determined"—e.g., the 11th century Jewish mystic Baḥyâ Ibn Paquda and Immanuel Kant; see Goodman, 1983.) Notwithstanding this ambivalence, most behaviorists and nonbehaviorists alike attribute a determinist point of view to behaviorists. I doubt, however, that most people subscribe to such determinism with respect to their own actions.

I hypothesize that "most people" are correct: Behavior is often not determined, and no matter how much information we scientists gain, we shall never reliably predict or control much significant behavior. This last hypothesis will be a red flag to radical behaviorists, and I hesitate to follow it with the goring bull of a Gedanken proof, but here it is. Imagine that you are trying to predict which flavor of ice-cream I shall choose in a series of choices between two flavors. I will grant you complete knowledge concerning my genes, conditioning history, and prior experience with ice-cream. You make a prediction and I make a choice. I assert that you will be unable to predict my choices at greater than chance level. It is, by the way, your—and everyone else's—inability that defines indeterminacy. I shall use a Geiger counter and ascertain the interval in which emission of an atomic particle is equally likely to occur or not. I then choose one or the other flavor depending upon whether or not the Geiger counter clicks within the interval. Your predictions will be no better than chance. This example is not trivial; many human choices can be, and sometimes are, based on chance occurrences. You may nonetheless object partly because if you knew what the Geiger counter was doing just before I did, then you could predict with 100% accuracy. I therefore shall briefly describe a self-experiment that demonstrates that presence of a Geiger counter is unnecessary.

At least one human subject—namely me—learned to emit numbers randomly in the absence of Geiger counters, coins, or any other external aid (Neuringer, 1980). I sat at a computer terminal, entered a series of 100 single-digit numbers as randomly as possible, and then received feedback from a PDP-1170 computer as to how random the series was according to a set of statistical tests. Numerous studies in the psychological literature have concluded that people cannot behave randomly under situations similar to this (Wagenaar, 1972). But in none of these studies did subjects receive feedback concerning their performance, and in none was long practice given. After considerable training, I learned to be "random"—that is, my performance could not be distinguished from that of a computer-based random-number generator, according to some 30 different statistical tests. People, I therefore hypothesize, can behave "randomly," that is, in ways that can not be distinguished from computer-based random distributions. (See also Page & Neuringer, in press.) Furthermore, I hypothesize that although behavior is influenced by environmental events, a constant environment will not necessarily engender constant behavior (my random numbers were emitted in a constant environment); or given complete knowledge— whatever that might mean—we will not necessarily have complete control over behavior, or be able to accurately predict behavior.

The process of operant shaping—in which reinforcement depends upon successive approximations to a specified response—requires, according to theory, initial variability of behavior (Skinner, 1938; Staddon, 1983). Without sufficient variation, there could not be successful selection. This conjecture might be extended: The underlying nature or micro-nature of the operant is indeterminate, nonpredictable or, if you will, free, and it is from this substrate that the environment—in the form of reinforcers—makes its selections. From this point of view, operant conditioning can be viewed as the intersection between deterministic selective processes and indeterministic variability-inducing processes. Perhaps behaviorists could do a timely about face and assert not only that freedom exists, but that operant conditioners have, in fact, been the primary explorers of freely emitted behavior. Behavioral researchers could profitably emphasize the important research begun by Antonitis (1951), Herrnstein (1961), and others and inquire into the functions served by variability in learning, problem-solving, and creativity; they could study how best to manipulate—increase or decrease—that variability (e.g., see Blough, 1966; Bryant & Church, 1974; Killeen, 1982; Pryor, Haag, & O'Reilly, 1969; Schwartz, 1982).

Whether the focus of the research is applied or basic, self-experimentation brings scientific methods to our own lives. It is the quintessence of the N = 1 method and it increases the probability that the research will be relevant—and therefore potentially meliorative—to at least one N. In the applied area, behaviorists might experiment on how to increase the effectiveness of their own verbal interactions: to avoid unnecessary arguments, to give and receive criticism wisely, and to increase the likelihood that others will listen (e.g., Lyons, 1976). Behaviorists are ideally suited to study the effects on their own health of nutritional, social, and reinforcemental variables. Self-experimenters could rejoice when thrust into a conflict situation, into fits of jealousy, or into emotional turmoil, for these provide the opportunity to explore conflict-resolution techniques, to discover new things about ourselves, and to report these to others. In the more basic arena, behaviorists might inquire as to the effects of self-presented reinforcers—on themselves. Or they could begin to explore rates of responding in novel and potentially useful ways. For example, response rate seems to be an important determinant of the aesthetic dimension of my response. That is, response rate serves as an important *independent* variable. When I look quickly across the ocean, it is just the ocean. When I look slowly, intently, moving my eyes ever so slowly, I respond to it as an incredibly beautiful ocean. So, too, with a work of art, or any common object, such as my hand. Try varying the speed of your behavior and observe the effects on your aesthetic response. Behaviorists need not be committed to any particular dependent variable, such as rate or probability; it may be more useful to increase the "beauty" of behavior than its frequency.

The key to survival is variability. Especially when the environment is changing rapidly and dramatically, there must be a variable substrate from which adaptive structures and functions can be selected. Self-experimentation increases the likelihood of variability within one's life, for variation is necessary when doing experimental research: The self-experimenter engages in controlled variations to learn more about him or herself and the environment. But self-experimentation provides variability of another form, for it varies the way we confront problems and the way we use science to make discoveries. It is just one alternative, of course, towards a more meliorative science of behavior.

> To the sick the doctors wisely recommend a change of air and scenery. Thank Heaven, here is not all the world. . . . The universe is wider than our views of it. . . . I left the woods for as good a reason as I went there. Perhaps it seemed to me that I had several more lives to live, and could not spare any more time for that one. It is remarkable how easily and insensibly we fall into a particular route, and make a beaten track for ourselves. I had not lived there a week before my feet wore a path from my door to the pondside; and though it is five or six years since I trod it, it is still quite distinct. It is true, I fear, that others may have fallen into it, and so helped to keep it open. The surface of the earth is soft and impressible by the feet of men; and so with the paths which the mind travels. How worn and dusty, then, must be the highways of the world, how deep the ruts of tradition and conformity! (Thoreau, 1854/1965, pp. 237, 239)

How many other lives might each of us live? You are probably sitting at this moment. Why not vary—try walking or dancing as you read? The clothes we wear are all rather similar: What recommends these garments? Can you learn to vary the quality and required amount of your sleep? Can you control your emotional responses? Ghandi was silent one day each week, Ezra Pound for much longer periods. How might periods of silence affect you? Your attention: Where is it focused? Behind your eyes, mouth, or ears? Can you change that? How does your ongoing covert patter, what you do in your head, compare with others? Is it unfair to compare such self-experimental questions with those from the highways of operant research?

> Experimentation, Mr. Castle . . . Experimentation with life—could anything be more fascinating? . . . let me ask you to compare what I am doing for world peace with what you are doing . . . What are your techniques? What progress are you making toward a peaceful life? (Skinner, 1948, pp. 145, 170)

Whether through interactions with applied behavior analysts, through the building of models relevant to extra-laboratory issues, or through self-experimentation, if basic behavioral scientists stepped toward meliorative change, their journey might help create more days to dawn.

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